In my judgment, this is only another and more emphatic way of stating the coordination of structure and function which has been insisted on by Prof. Owen and other naturalists again and again. In the first passage that I have quoted all this dependence of structure on "conditions" is assumed to be true. In the second passage, assuming it to be true, it is generalized into a law. In the third passage, assuming the existence of the law, its results are assumed to be tolerably uniform.

Now I am not aware that any number of assumptions, vague ideas, or guesses will make a discovery; and if they had done so, are we not entitled to assume that the discoverer, instead of publishing it anonymously, in a few vague sentences at the end of a review in a specially professional periodical, would have avowed his great thought, and brought it prominently before naturalists who could judge of its value? especially as he is now anxious to have credit for it.

I have also had an opportunity of referring to the 'Principles of Biology;' and although Mr. Spencer insists with admirable clearness on the correlation of structure and function, and, as in the review, on the modification of structures by "incident forces," I did not notice that these "incident forces" were defined; while, so far as I could understand, Mr. Spencer confessed that he did not altogether see how their results were produced.

If this is a correct statement of Mr. Spencer's vague hypothesis, I submit that, but for the terms "pressure and tension," and "mechanical theory," our views have little in common. His appears to me to have been an idea evolved out of an intellectual consciousness of what ought to be. My view was arrived at inductively from a long investigation; and it was only when I was assured by mathematicians, chemists, physicists, and others of their willingness to cooperate in eventually demonstrating the view, that I consented to publish a sketch of my method of studying the theory of the skeleton. For it is a part of a larger system referring the phenomena of nature to their ultimate and actual physical causes, many of which in their applications to life are discussed in a book of mine shortly to be published, on "The Dynamical Geology of Great Britain."

I am, Gentlemen,
Very faithfully yours,
HARRY SEELEY.

Note on the Phenomena of Muscular Contraction in the Vorticellæ. By C. ROUGET.

Living muscles can alternately shorten and elongate themselves: this is their characteristic property. In purely elastic organs shortening only takes place after previous mechanical elongation; the muscles, on the contrary, can shorten themselves without appearing to have undergone any extension.

Whatever may be the causes of the elongation and shortening of the muscular fibres, whether these opposite states result from a mechanical extension followed by retraction, or whether they are produced apparently spontaneously, observation proves that in either case the alternate changes which the contractile organ undergoes are identical. In a muscular fibre which, after mechanical extension, returns upon itself in virtue of its elasticity, the transverse striæ change their aspect and approach each other, at the same time that the transverse diameter increases in proportion to the diminution of the length. It is exactly in the same way that the muscular fibre behaves in passing from the state of elongation corresponding with the repose of the muscle to the state of *active* shortening designated by the name of *muscular contraction*. If the essential phenomena by which muscular contraction is manifested are identical with those of the elastic contraction of muscles—if, on the other hand, the elementary structure of contractile organs appears specially adapted to the manifestations of elasticity, we may justly ask whether it is necessary to invoke, in order to explain the shortening of muscle in the state of contraction, a special property of contractility, distinct from the properties of inorganic matter.

Elasticity may become a cause of movement in two opposite conditions :—

Either the elastic body, the spiral spring, is subjected to a pressure which keeps the turns of the spiral in a forced approximation, when, on the pressure ceasing, the turns separate, the spring elongates and moves by the mere fact of its elasticity ; or the spring is subjected to a tension which elongates it by separating the turns of the spiral from each other ; on the tension ceasing, the turns approach each other, and the spring moves by shortening, without anything but elasticity coming into play.

The alternations of elongation and shortening of the elastic elements (*spiral fibrillæ*) of the muscles might therefore be explained by elasticity alone, if we demonstrated the existence either of an agent of pressure exercising its action during the period of shortening, or of an agent of extension acting during the period of elongation—the muscle elongating in the former case and shortening in the latter by the free play of elasticity the moment the action of an antagonistic force ceases to equilibrate it.

The physiological problem of muscular movement is thus brought to its most simple terms—to determine the natural form (the state of repose) of the muscular spring, the conditions which can remove it therefrom, and those to which elasticity recalls it.

There are at present two hypotheses as to the cause of muscular movement : one attributes this movement to a special property of muscular fibre, *irritability* or *contractility*, which manifests itself only in the period of activity of the muscle and produces the shortening ; the other, on the contrary, regards the shortening as the return of the muscle to a state of repose. This latter hypothesis, which supposes that, during the period of apparent inactivity of the muscle, the nerves are constantly at work to maintain the forced extension of the contractile fibres, is certainly refuted by the incontestible fact that the section of the motor nerves does not cause the contraction of the muscle, but, on the contrary, the opposite state; nevertheless it approaches the truth much more closely than the former.

The observation of the phenomena of muscular contraction is presented to us in the *Vorticellæ* in the most simple condition which it is possible to imagine. In many Invertebrata an entire muscle is often represented by a single *primitive bundle*; in the Rotifera isolated fibrillæ form so many distinct muscles. The stalk of the *Vorticella* shows us the principal organ of locomotion of an animal composed of a single muscular fibrilla free in a canal in the centre of a perfectly transparent sheath, which allows us to see, with the greatest distinctness, all the changes which the contractile element undergoes during the states of activity and repose, of elongation and contraction.

When the animal is at rest, the stalk is at its maximum of elongation, and the body as far removed as possible from the point of attachment and refuge. The vibratile cilia alone are active, the body and the stalk remain perfectly immoveable. In this state the central filament of the stalk, the contractile fibrilla, is completely extended; nevertheless it is never straight, but constantly presents a torsion in a very elongated spire, like a ribbon twisted round its longitudinal axis, and of which the appearance exactly resembles that of a spiral watch-spring fixed and strongly extended by the extremities. As soon as any mechanical, electrical, thermic, or other excitant affects the animal, this elongated spiral, suddenly contracting upon itself, becomes transformed almost instantaneously into a perfectly regular spiral spring, with very close turns, which does not measure more than one-fifth of the length of the stalk in repose, and of which the transverse diameter has proportionately increased. This state generally persists only for a short time: the turns of the spring separating, it soon elongates, rather slowly, and the animal returns to its natural position.

The shortening and elongation of the contractile organ are here manifestly due to the approximation and separation of a spiral spring; but to which of these two states belongs the action of elasticity? which of them shows us the muscular spring in its natural form, in its state of repose? Observation establishes, in the first place, this important fact—namely, that the spiral filament never appears in its extreme elongation except when the animal is alive and uninjured. As soon as the animal is killed, or detached from its stalk, spontaneously or by violence, the turns of the spiral roll themselves up like a tendril, and remain in this state for an indefinite period; the same is the case if the animal be suddenly killed by poison or by the elevation of the temperature to 104° or 113° F.

It frequently happens, even during the life of the animal, that the contractile fibrilla breaks, and the continuity is broken between it and the body, the nutritive centre of the whole animal; in this case, if the sheath be intact and continuous, the body, living and swimming by means of the vibratile cilia, drags along at its posterior part the dead contractile fibrilla rolled up like a tendril, persisting in this state of contraction, and having lost for ever the faculty of elongation.

I have several times observed that as soon as the body of a *Vorticella* detaches itself from the stalk to which it normally adheres, the contractile stem begins to execute a series of movements of rotation round the axis. Each of these movements is accompanied by the formation of a spiral turn; and when the whole of the stalk is thus converted into a close spiral, the movement ceases, and no elongation afterwards takes place.

The elongation of the spiral fibrilla, the organ of muscular movement in the *Vorticellæ*, is therefore dependent on the state of life—that is to say, on the continuity of nutrition and the exchange of materials. From the moment when nutrition is suppressed by the death of the animal, or by the separation of the fibrilla from the nutritive centre, the contractile element takes and retains the natural form inherent in its structure—that of a spiral spring, of which the turns are at the maximum of approximation in the state of repose.

The contraction of the muscular fibre of the stalk of the *Vorticella* corresponds with the state of repose of the spring; it is the immediate consequence of its elasticity; the elongation of the fibre is the result of the forced extension of the spring by a cause of movement dependent on the act of nutrition, and acting during the apparent repose of the contractile organ. As soon as the source of this antagonistic force is exhausted, elasticity, recalling the muscle to its natural form, produces the so-called movement of contraction.

Is this a phenomenon peculiar to a singular organ of locomotion, the stalk of the *Vorticella*? or is it the condition of muscular contraction in all animals?

I shall have the honour very shortly to communicate to the Academy the results of numerous experiments which I have undertaken upon muscular contraction in the higher animals, their results establishing:—

1. That a recent hypothesis, according to which permanent contraction is essentially constituted by a series of successive shocks or vibrations, is in absolute contradiction to well-observed facts.

2. That a tendency towards extreme contraction is a property inherent in living muscular fibre, a necessary consequence of its structure and elasticity.

3. That during life this tendency to contraction is combated by a cause of extension which predominates during the repose of the muscle, is developed in the exchange of nutritive materials, increases with the activity of their access, diminishes or becomes extinguished by their exhaustion, and may be momentarily suspended by all the excitants of muscular contractility—nervous action, heat, the electric shock, &c.—*Comptes Rendus*, June 3, 1867, pp. 1128–1132.